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THE PRESENT STATUS OF THE GENETICS PROBLEM [1]

THE problem of heredity has been attacked in four principal ways. Galton developed to a high degree what we may call the statistical method. His most important conclusions are embodied in his law of ancestral inheritance and his law of regression. According to the former, the two parents together contribute one half of the total inheritance of an individual, the four grandparents one fourth, the eight great-grandparents one eighth, and so on indefinitely. The law of regression attempts to state the average deviation of a fraternity from the mean of the general population in terms of the average deviation of the two parents. Recent investigations have shown that neither of these laws is true except for averages of large numbers of cases, and not in all cases even then. They are not applicable to individual cases, and are hence of no importance in the modern science of genetics, however important they may be in statistical problems in general.

In recent years the methods used by Galton have been developed by Pearson and others into a highly mathematical treatment of the subject of heredity, which has given us important means of dealing with the precision and reliability of data and enabled us to study certain types of correlation to advantage, but which has otherwise had comparatively little influence on the progress of genetics. The study of correlation between hereditary characters by statistical methods has not as yet led to

[1] Presidential address before the Washington Botanical Society, March 5, 1912.

discoveries of any great usefulness in the work of producing new and improved races of plants and animals, however useful it may have proved in other directions, or may yet be in genetics.

Another method of study has been that of the cytologist. A long list of able investigators have in recent years given attention to the phenomena of cell division, especially the process by which gametes, or reproductive cells, are produced. Very soon after the rediscovery of Mendel's principles cytologists pointed out that the behavior of the chromosomes in the reduction division is sufficient to account for Mendelian phenomena if a proper connection between the chromosomes and Mendelian characters could be proved. One of the most important results achieved in this line of investigation is the demonstration of a relation between certain chromosomes and the determination of sex. The work of Professor E. B. Wilson has been especially convincing in this respect, though many other investigators, especially Professor Morgan and Miss Stevens in this country, and Boveri, Baltzer and others in Europe, have contributed important results. The net results of these investigations are that in most species the female possesses a pair of chromosomes of peculiar character, usually distinguishable from the other chromosomes, and, because of their behavior in the prophases of the first maturation division, called by many cytologists "idiochromosomes." The male has only one of this type of chromosome. From the behavior of these idiochromosomes it results that all the eggs contain one idiochromosome, while the sperm is of two kinds, one containing a single idiochromosome, the other none. Eggs fertilized by the former produce females; by the latter, males. In a few species the female has only one of these peculiar chromosomes, while the male has none. In these species the female produces two kinds of eggs, one female-producing, the other male-producing; while the male produces only one kind of sperm. These investigations have thus given strong reasons for believing that sex is an inherited character, and is hence not determined by external conditions.

These facts have been demonstrated for a large number of species (over a hundred), including man.

While in most species the male possesses but one of these presumably sex-determining chromosomes, it frequently happens that this chromosome has a synaptic mate, which, however, appears not to be concerned in sex production. This synaptic mate consists in some cases of a single chromosome; in others it consists of a group of chromosomes varying in number from 2 to 5 in different species; in still others it is wanting entirely. These facts are of special interest in connection with the further fact that a large number of ordinary somatic characters have been found to form Mendelian pairs with the sex element. Thus, in barred Plymouth Rock poultry the barring of the feathers is transmitted by the female only to her male offspring. Many human affections are transmitted in a similar manner, such as night blindness, color blindness, etc. Pearl has shown that high egg-laying quality in poultry is similarly sex-limited. Females do not transmit this quality to their daughters, but do transmit to their sons the power of transmitting to the granddaughters high egg-laying quality. A long list of such sex-limited characters has been demonstrated. These facts raise the presumption that these sex-limited characters are related to the chromosomes—in what manner of course we do not know.

On the history of the chromosomes during the life history of the cell, especially

during the events immediately preceding the metaphase of the reduction division, cytologists are somewhat divided on matters of fact. They are in essential agreement so far as the genetic continuity of the chromosomes is concerned, *i. e.*, they believe that each chromosome is directly descended from a previous one. There are cases, however, where the propagation of the chromosomes is not a simple matter of division in which all the substance of the old body passes directly into the two new ones formed from it by division.

The manner of formation of bivalent chromosomes in the early stages of the reduction division is still a matter of dispute, as is also the possibility or probability of exchange of substance between synaptic mates. A review of the work on this subject would be highly interesting, but would extend this paper beyond all reasonable limits.

Recent work has shown that many chromosomes are really compound bodies, and that what we have regarded as independent chromosomes are, in some cases at least, really groups of chromosomes. This fact may have an important bearing on partial or even complete correlation between hereditary characters, as well as the fact that in some species more Mendelian pairs have been demonstrated than there are pairs of chromosomes.

A third type of investigations relating to heredity is the study of the chemical processes concerned in development. A great deal of the work of the physiological chemist has a bearing on this subject, though not instituted directly for this purpose. This study has led to the conclusion, as Guyer states, that at least a principal function of the chromatin is the production of enzymes which, by their regulating effects on metabolic processes, produce important effects in the development of the organism. Recently Gortner, of the Carnegie Institution, and Miss Wheldale, of Cambridge University, have attempted to work out the chemistry of certain of the pigments, with important results. For instance, Gortner has shown that, in the potato beetle, oxidizing enzymes are generally present in the body, while chromogen, which these enzymes convert into pigment, is produced locally in the integument in small quantity, and only in those locations where the characteristic pigment spots occur. One of our own members, Dr. Bartlett, is now doing some interesting work on the chemistry of anthocyan, and has become greatly interested in this phase of heredity investigations. Such investigations relate, of course, to the manner in which the hereditary characters make their appearance in development, not to the transmission of these characters.

This phase of heredity investigations is, as yet, only in its infancy. It is, however, one of the most important at the present time, for we can not go a great deal further in the interpretation of the phenomena of heredity until we know more of the chemical processes involved in the development of a complex organism from the fertilized egg.

Lastly, we come to the study, by experimental cross-breeding, of the behavior in inheritance of the various characters which distinguish nearly related organisms from each other. If relative importance were to be judged solely by the amount of data accumulated, this would be by far the most important phase of the subject before us. Here we have a plethora of fact and a dearth of meaning. We have now reached a point in the study of Mendelian inheritance where satisfactory interpretation of fact is quite as important as the collection of more facts. We need stimulating theories that will point out new directions for

research. When Mendel's laws were rediscovered practically simultaneously in the closing year of the last century, by de Vries, Correns, and von Tschermak, biology received such a stimulus as it has not felt since the publication of the "Origin of Species" in 1859. But what was it that gave this stimulus? Was it the collection of facts? Was it the wonderful collection of facts regarding variation made by Darwin that electrified the scientific world in 1859? Or was it a satisfactory interpretation of these facts? Darwin gave his facts *meaning*. Similarly, it was an *illuminating interpretation* of facts that made an epoch in the development of biological science when Mendel's principles were rediscovered. The fact that this discovery lay unnoticed for a third of a century and then suddenly became the leading interest of biologists is a remarkable commentary on the relation of science to human welfare in the last century and in the present.

The leading principle discovered by Mendel was that a hybrid whose parents differ in respect to a single factor of development, produces two kinds of gametes, respectively like the gametes of the two parents. This is now known as the law of segregation. In the early years of the present century genetic investigations dealt mainly with the universality of this law. This question is now practically settled, so far as can be by experimental cross-breeding. We now have a vast amount of data which need further interpretation that will point the way to new *kinds* of facts. The mass of data which has accumulated during the past dozen years has been variously interpreted by different investigators. It has furnished an extensive vocabulary of new terms, to which various meanings have been attached. Some recognized authority has suggested a new view concerning the nature of the so-called "unit-characters," and this view has been widely accepted with no critical examination of its intrinsic merits. The weight of authority here, as elsewhere, has been an incubus on the progress of scientific interpretation.

Mendel himself did not make use of the term "unit-character," but refers the phenomena he observed to *differences between formative elements* in the cells of hybrids. This is made clear in the following quotation from his original paper:

> With regard to those hybrids whose progeny is variable we may perhaps assume that between the differentiating elements of the egg and pollen cells there occurs a compromise, in so far that the formation of a cell as foundation of the hybrid becomes possible; but nevertheless the arrangement between the conflicting elements is only temporary and does not endure throughout the life of the hybrid plant. Since in the habit of the plant no changes are perceptible during the whole period of vegetation, we must further assume that it is only possible for the differentiating elements to liberate themselves from the enforced union when the fertilizing cells are developed. In the formation of these cells all existing elements participate in an entirely free and equal arrangement, in which it is only the differentiating ones which mutually separate themselves. In this way the production would be rendered possible of as many sorts of eggs and pollen cells as there are combinations possible of the formative elements. . . . The differentiating characters of two plants can finally, however, only depend upon differences in the composition and grouping of the elements which exist in the fundamental cells of the same in vital interaction.

Thus instead of "unit character" Mendel speaks of "differentiating characters," and instead of pangenes in the germ plasm he speaks of "formative elements," differences in which are responsible for the differences in related organisms. Nowhere does he advance the idea that the germ plasm is composed of independent elements, each of which is responsible for the development of a definite portion of the organism. The latter idea is due to de

Vries. Until within the last two or three years the de Vriesian interpretation of Mendelian phenomena has been widely accepted, especially in England and in Germany. Generally speaking, American biologists have hesitated to accept the de Vriesian doctrine, preferring to regard the developed organism not as a structure composed of definite elements independent of each other in hereditary transmission, but rather as a complex resultant of the interaction of various cell elements no one of which is wholly responsible for any definite part of the organism.

In consonance with the de Vriesian conception, the idea early developed that the organism is a collection of "unit characters" arranged in pairs, any one of which might be replaced by certain others. Bateson, in 1901, in presenting to the Royal Horticultural Society a translation of Mendel's original paper, uses the following words:

> In so far as Mendel's law applies, therefore, the conclusion is forced upon us that a living organism is a complex of characters, of which some, at least, are dissociable and are capable of being replaced by others. We reach thus the conception of unit-characters, which may be rearranged in the formation of the reproductive cells.

This is the first use of the term unit-character the writer has been able to find. The idea that hereditary characters are indivisible units is, however, due to de Vries. In de Vries's original paper on the law of segregation he remarks:[2] "According to the principles which I have elsewhere announced (Intracellular Pangenesis), the specific characters of organisms are composed of units quite distinct"; and, again, "for the simple character must be considered as an individual unit." The term "unit-character" did not come into general use until about 1905 or 1906. The fact that it presented a conception easily apprehended, and the further fact that this conception lends itself readily to a convenient system of symbols for representing the phenomena concerned, led to the rapid adoption of the new phraseology even by those who reserved their opinion as to the philosophy on which the idea was based.

We have already seen that Mendel himself referred the phenomena he observed to differences in formative elements in the germ plasm, which were "in vital interaction." The de Vriesian philosophy, on the other hand, did not place much stress on this vital interaction, but looked upon each hereditary character of the organism as the expression of a particular element in the germ plasm which was, more or less independently of all others, responsible for the development of that character. A pair of segregating characters, such as smoothness and wrinkling in pea seeds, was looked upon as due to a corresponding pair of pangenes in the germ plasm, one of which, if allowed free action, would produce smoothness, the other wrinkling. Bateson's term allelomorph was generally used to refer to these hypothetical organs in the germ plasm, rather than de Vries's original term pangene, since the former term could be applied to the visible characters themselves as well. Later, when certain non-contrasting characters were found to segregate from each other, Bateson gave this phenomenon the name "spurious allelomorphism." An example already given of such a pair of segregating characters which are not contrasted characters is femaleness and the barring of the feathers in Plymouth Rock fowls; this means that when the reduction division occurs one of the resulting cells carries the potentiality of femaleness, the other that of producing bars on the feathers. Many other instances

[2] I am indebted to Dr. Geo. H. Shull for the following citations relating to the history of the unit-character conception.

of such pairing of unrelated characters are known in both plants and animals.

About 1903 a new conception of the pair of allelomorphs arose. In that year, both Correns and Quenot foreshadowed what was more definitely suggested by Bateson and Punnett in 1905, as the "*presence and absence hypothesis.*" C. C. Hurst the next year developed this hypothesis to its logical conclusion.[3]

According to this hypothesis the difference, say, between two varieties of cowpeas, one of which has red and the other white seed coats, is due to the presence of an allelomorph for red in the one case and its absence in the other. When the reducing division occurs in the hybrid this allelomorph was supposed to pass entire into one of the daughter cells, thus giving two types of gametes, one with, the other without, the potentiality of producing red pigment in the seed coat. It is usually assumed that the dominant member of the character pair corresponds to the presence, and the recessive member to the absence, of an allelomorph, though there are apparent exceptions to this rule.

While some biologists still adhere to the idea that the organism may be regarded as a structure built up of parts each of which represents a separately inherited character and is represented in the germ plasm by a pangene, this idea is much less prevalent than it was a few years ago. Recently the attempt has been made to formulate a description of Mendelian phenomena in terms that do not involve any hypothesis concerning the nature and interaction of the germ plasm elements which are certainly responsible for these phenomena. Notable amongst such attempts is the "genotype-conception" of Johannsen. Johannsen does not define very definitely his term "gene," further than to apply it to the cell organs or cell substances, whatever they may be, that are responsible for Mendelian phenomena. He particularly insists that the gene is not to be regarded as the basis in the germ plasm of a particular character, but that Mendelian phenomena arise from differences in corresponding genes in two varieties. This idea is in close accord with that of Mendel, which we have already discussed. Johannsen's gene and Mendel's formative element appear to be the same thing. Any particular character is probably the result of the interaction of several or many genes, and any one gene may bear a relation to many characters.

[3] I am also indebted to Dr. Shull for these historical facts concerning the presence and absence hypothesis.

In a field of investigation where so much confusion has existed as to the meaning of terms, and where widely different views have been maintained as to the significance of the phenomena observed, it is necessary, in order to render discussion fully intelligible, to describe quite accurately the facts, which are not in dispute, and thus attach definite meanings to the terms used. It is not so important that we should agree as to the proper use of terms as that we should understand clearly the actual meaning of a writer, whether he uses terms correctly or not. In order that the meaning which I attach to the term "Mendelian factor" may be made clear, I shall set forth in some detail the facts about a group of these factors with which I have been working for some years past.

In my investigations of heredity in the cowpea (*Vigna unguiculata*) the seed-coat colors found are as follows: white (or cream), red, buff, blue, brown, black and purple. In addition to the simple colors above enumerated, certain varieties have the surface more or less thickly covered with bluish purple dots (speckling). In

the New Era variety these dots are very numerous, while in the variety known as Taylor the dots are more thinly scattered, with considerable areas free from them. Certain other varieties, notably the whippoorwill, are characterized by a peculiar mottling of the surface in which irregular areas of darker shade are separated by lighter areas, apparently of the same coloring material, but less dense. There are also two genetically distinct types of "eye," the data concerning the inheritance of which have already been published.[4] Finally, certain varieties are characterized by irregular longitudinal stripes on the sides of the seeds. We have thus, in all, 13 pairs of Mendelian characters relating to the seed coat.

Except the purple color, the inheritance of all the above characters has been worked out quite completely. Purple was introduced into my crosses from a variety that had only a small color patch about the hilum, and was not recognized as a distinct color type until after the crosses had been made. It happened that the crosses made were not such as to bring out in full the relations of this purple color to the other Mendelian factors involved. Its inheritance will be investigated later.

Without postulating anything whatever as to the nature of the Mendelian factors involved in the development of these colors, I will first set forth some of the more important facts that have been worked out concerning which factors must be present in order that a given color may develop. Since we do not know what these factors are, and hence can not give them names based on their nature, and since we do know their behavior in inheritance and the relation of their presence and absence to the development of the colors, I shall give them provisional names based on their behavior and their relations to color development, and then, for convenience, reduce these names to mere symbols.

In no case is there a single Mendelian factor that can by itself give rise to colored seeds. I have been able to demonstrate that red, buff and brown seeds each require at least two factors. They may require others, but if so these others have not been detected. Black requires three factors and blue three.

If all these factors were distinct, these five colors would thus require twelve separate factors for their development; but the whole number of these factors for color (omitting purple) is only six. The same factor takes part in the development of more than one color. In fact, there is one that is common to the whole series, and when this factor is absent white seeds result, even if the remaining five are all present. We may designate this factor as the general color factor, and represent it by the symbol C. If we represent the other five factors by the letters R (red), U (buff), Br (brown), B (blue) and N (noir = black), the conditions required for the production of each color in this series, as shown in the breeding experiments, is shown in the following table:

CONDITIONS REQUIRED FOR DEVELOPMENT OF COLORS

Colors	Factors Necessary	Factors Having no Effect	Factors Which Must be Absent
White		R, U, Br, B, N	C
Red	C, R	N or B	U, Br, B or N
Buff	C, U	R, N or B	Br, B or N
Brown	C, Br	R, U, B	N
Black	C, Br, N	R, U, B	
Blue	C, N, B	R[5], U[5]	Br

In this table the names of the colors are given in the first column. In the second column are given the factors that must be

[4] *Amer. Nat.*, Vol. XLV., No. 537, September, 1911, pp. 513–24.

[5] These do not affect blue, but are visible with blue.

present in order that a given color shall appear. Thus none of these six factors is necessary to the production of white seeds. For red seeds there must be present the factors *C* and *R;* and so on. It will be noticed that the factor *C* is necessary in all the colored types.

The third column shows the factors whose presence or absence is immaterial in each color type. Thus white seeds may or may not possess any or all the factors except *C*. Judging from results secured by others in other species, there could also be white seeds possessing the factor *C,* provided all the other factors concerned in color production were absent, but thus far I have not found white seeds of this character in cowpeas.

In the case of red and buff seeded varieties it will be noticed that *N* or *B* may be present, but they may not both be present. The reason for this is that *C, N,* and *B* are the factors for blue; hence if both *N* and *B* were present in red or buff varieties these would be converted into blue. Opposite blue, in the third column, *R* and *U* are given as factors that may be present without modifying the blue color; while this is true, it is also true that these colors (red and buff) can be seen along with the blue in blue seeds. This seems to be due to the fact that blue is a sap color, and is usually not very intense, while red and buff are pigments in granular form. It is interesting to note at this point that these surmises of mine as to the nature of these colors have been confirmed by Dr. Mann, to whose work with these pigments I shall refer later.

The last column shows factors that must be absent in the various color types. Thus, if *C* is present, white seeds do not occur, unless indeed all the other factors are absent as well. The factor *U* can not be present in red seeds, for it would convert them into buff. In general, omitting blue, each of the colors named in column one completely conceals, or possibly prevents the development of, all those above it. Hence the factor *Br* can not be present in red and buff seeds. As already stated, the factors *B* and *N* can not both be present in red seeds, and the same is true of buff, though either of them may be present if the other is absent. The factor *N* must be absent in brown seeds, for otherwise we should have all the factors for black, and black renders brown invisible. Similarly, the factor *Br* may not be present in blue seeds, since it would change their color to black.

All the factors mentioned in this table are fully demonstrated in the experimental work, the full data of which will be published later. Not only that, but these factors, whatever they are, can be shuffled about and combined in any manner desired, practically at will. There are no intermediate stages to be dealt with. A factor is either present or not present, and that is the end of it. The only difficulty that presents itself in attempting to produce any color desired lies in the indifferent factors shown in the third column of the table. In some cases it is necessary to test a variety by appropriate cross-breeding in order to ascertain what characters are concealed in it, before we know what to expect when it is crossed with certain others. This will be necessary, for instance, in order to ascertain whether a brown variety possessed the factors *R* and *U*. It would not be necessary, however, to test any variety having colored seeds in order to learn whether it contains the factor *N,* for if *N* is present the green parts of the plant will exhibit anthocyan, which they will not do if *N* is absent. To learn whether a white variety contains *N* we should cross it with brown, in which case

the hybrid would be black or brown according as the factor *N* is or is not present.

This factor, to which I have given the symbol *N*, and which is necessary to the production of both blue and black seed-coat colors, is, as already stated, one of the factors for the production of anthocyan in the green tissues, and this was one of the reasons which led me to suspect that the blue pigment is a sap color. This same factor *N* is also one of those necessary for color in the flowers. But its importance in the economy of the plant does not seem to stop here. It may be merely a coincidence, but amongst about 4,500 third-generation plants grown in my experimental plots last year, this factor was present in every one of them that made even a fair yield of seed, except one strain of browns, and, with this exception, in those plants which lacked this factor the yield of seed was very meager. Furthermore, it is present in every standard variety of cowpeas in this country, so far as I have observed, although many varieties have doubtless been produced that did not possess it. These facts would seem to indicate that this factor is in some way connected with vigor and vitality in the plant.

Regarding the above colors, my breeding experiments led me to think that red, buff, brown and black were pigments related to melanin, and that blue was a sap color related to anthocyan. Dr. Albert Mann has made a study of this matter and has secured interesting and important results, which he will publish later. He kindly permits me to state that these surmises of mine were correct. I mention this fact to show that breeding experiments may be helpful to the chemist in investigating the chemical nature of these pigments.

We now have before us some of the facts that so badly need interpretation. The case is not nearly so simple as this statement of facts would indicate, for there are other important classes of facts that I have not considered because to do so adequately would require too much space. It is not surprising that such facts as these have led to much controversy. It is inevitable that the human mind shall attempt to comprehend what it clearly apprehends, and hence that theories of various kinds should have been proposed to explain these facts. When we consider the fact that these factors can be shuffled and recombined in every possible way, just as if they were concrete entities, each represented by an independent morphological element in the germ plasm, it was to be expected that theories should be proposed involving such elements. It is inevitable that this should have been the case. Such theories present a simple idea, easily grasped, leading to an almost absurdly simple scheme of symbols for portraying the facts of segregation and recombination.

Some of these theories have gone so far beyond the present possibilities of investigation that many biologists, especially those only slightly familiar with the facts of Mendelism, have entered vigorous protests, and have even gone so far as to try to rule the facts themselves out of court. I wish to suggest to those who have not themselves conducted Mendelian studies, that it is well not to be too dogmatic about the facts in a field of investigation with which one is not very familiar. It would be a serious matter to convince any one who has watched the shifting and recombining of these factors that they are not real things. For one not thoroughly familiar with Mendelian phenomena to question the *facts* of segregation and recombination is as unseemly and unscientific a procedure as it would be for me to question the facts of physiological chemistry. The real trouble is not with the facts. It is with the interpreta-

tion of these facts. Just at present we have more facts of a certain kind than we know what to do with. We need some one to put meaning into these facts. We are in the position of a man lost in a wilderness. What he needs to find is a road. It does not make so much difference where this road shall lead, for all roads lead into each other. If he can find any road, it will lead him to where he can find people, and these can point out other roads leading more nearly in the direction he wants to go.

In genetic investigations we need theories that will suggest lines of investigation that will be fruitful of results—that will lead, not to more facts of the kind we already have, but to new kinds of facts that will throw light on the subject from a new angle. "Theories," said Pasteur, "come into our laboratory by the bushel. When they have served their purpose, they are thrown out of the window." This has been so in the development of the science of genetics, but just at present the supply of theories is almost exhausted.

The things that need interpretation are the manner in which segregation and recombination are brought about, the nature of the things that segregate and recombine, and their relation to the processes of development. Mendelian factors, that is, those factors of development that behave in Mendelian fashion in heredity, of necessity relate only to those differences that exist between organisms that are closely enough related to cross-breed with the production of fertile progeny. In certain species crosses, and in some other cases in which there is reason to suspect either a diseased condition of the cytoplasm, or a departure from the normal behavior in gametogenesis, cases have been found in which the factors of development do not segregate and recombine in the simple manner represented by the Mendelian formulæ. Aside from these cases, it seems a fair inference from the results thus far obtained that the differences between organisms sufficiently related to permit of cross-breeding with the production of fertile offspring which reproduce sexually in a normal manner, are universally subject to the laws of segregation and recombination. There is one quasi-exception to this statement; there are certain factors that, instead of segregating in the usual presence-absence fashion, segregate from each other, so that they can not reside permanently in the same pure-breeding line. In my cowpea investigations I have found a set of three such factors; when only one of these is concerned in a cross we get the usual phenomena of presence-absence segregation. But if any two of the three are brought together in the same zygote they segregate from each other. These three factors are the factor *Br*, above mentioned (the special factor for brown pigment in the seed coat), and the New Era and Taylor types of speckling.

The question whether the deeper and more fundamental characters of the organism, such as are concerned in the differences between organisms widely separated in the organic world, are inherited in Mendelian fashion is purely academic and of no practical importance either to the theory of heredity or the practise of the breeder, for this question can never be submitted to experiment, nor could the most definite knowledge on this point be applied in the production of new and improved races of plants and animals.

At present Mendelists are plodding along practically without working theories. Let us hope that some of them will stumble on to facts of a new kind that will give meaning to those we already have.

Personally, I am of opinion that the chemistry of the pigments is a field that is of great importance to the theory of

heredity. The facts regarding the inheritance of color are better known than is the case with other groups of characters. The suggestion that the "factors" above mentioned are merely the power of producing certain chemical substances seems to me to be worthy of consideration. I believe that on this suggestion a new working theory of inheritance can be constructed that will explain the facts without recourse to the idea of "unit-characters," "pangenes," or any kind of character "bearers," in the sense of bodies or substances which are alone responsible for the development of a given character. It would be out of place here to attempt to present such a theory. I think, however, that an illustration of what I mean may be permissible. Let us suppose that a series of wireless stations, say in San Francisco, Denver, St. Louis and Washington, attempt to relay a message from the Pacific to the Atlantic. Now, if the St. Louis station should fail to do its part, the message would not arrive. In this case, we may not say that, if this station had done its part, it would have been *the* sender of the message. The relaying at that station is merely one of a chain of events that are necessary to the success of the experiment. But if St. Louis fails, then that station *is* responsible for the *failure* of the message to arrive at Washington.

The production of red pigment in the seed coat of the cowpea may possibly be a complex process in which every part of the living substance of the cell is concerned; but if a single cell organ which performs a necessary part of this process fails to play its part, then red pigment fails to develop. The cell organ whose failure to perform a usual function may thus be accountable for the lack of pigment formation might then be considered the "gene," as Johannsen calls it, for the *absence* of red pigment; while the whole organism might be the gene for the presence of this pigment.

In order that the study of the physiological chemistry of pigment formation shall give results of the greatest importance to the theory of heredity, it would be an ideal condition if such work could be carried out by one who possesses a wide acquaintance with the facts and theories in both these sciences. But such men are not plentiful. The next best scheme would be a cooperative study of the subject by two men, one in each field. I am happy to be able to say that Dr. Bartlett has consented to take up the chemical end of this work in connection with my investigations of the genetic phases of the problem, and Professor Piper has promised to grow the material for such study. Gortner's important work in this field, in connection with Dr. C. B. Davenport's work on genetics, has already been mentioned. With all these investigations, and those of Miss Wheldale and Professor Bateson at Cambridge, it is to be hoped that we shall gain a somewhat more definite view of the nature of Mendelian factors.

W. J. SPILLMAN

THE PERUVIAN EXPEDITION OF 1912

THE Peruvian Expedition of 1912, organized under the auspices of Yale University and the National Geographic Society for the purpose of carrying on geographic and anthropologic exploration in Peru, will endeavor to continue and extend the work of the Yale Peruvian Expedition of 1911, utilizing the discoveries made then and continuing further along the same lines.

It is our purpose to pursue intensive studies in the region where reconnaissance work was done on the last expedition, taking advantage of the discoveries then made to guide the plans for this year.